

UNIVERSITY OF MICHIGAN

ANN ARBOR

DEPARTMENT OF ZOOLOGY

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Dr. Joshua Lederberg
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Dear Dr. Lederberg:

Thanks for the reprints. The recent review article was especially interesting and useful to me, since I have been floundering around in the maze of facts in this field for some time, trying to make some kind of sense out of them. I found much new and valuable information in the review, but would like to offer some criticisms of the section on insect symbioses.

Many of the difficulties reviewers are having in this field arise, in my opinion, from the uncritical compilation by Steinhaus (Insect Microbiology). To illustrate with an example dealt with by you, take the important work of Wigglesworth and Brecher on Rhodnius. From the account given in Steinhaus (pp. 234-235), the reader would gather that there are intracellular symbionts in this bug which Wigglesworth at first thought to be transmitted via the egg, but later (1944) turned out to be picked up from the environment by the nymph. However, the 1944 paper of Brecher and Wigglesworth definitely states that the intracellular particles earlier thought to be the symbionts (figured by Steinhaus on p. 234) were only "rows of cell granules", and that the actinomycete important in nutrition is found in the gut lumen only. Thus, the example of Rhodnius does not belong in the field of endosymbiosis.

It seems to me that the term endosymbiosis is meaningless unless it is restricted to the intracellular forms. Probably every animal has microbes living in its digestive tract, and it is inevitable that these provide components of the animal's nutrition. But this has no bearing on the problem of cell function, and one cannot keep the facts in order unless one consid

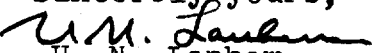
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considers the supposed intracellular microbes only on their own merits. It is of course possible that there is a broad transition between gut-dwelling and intracellular "symbionts", but this has not been demonstrated to my satisfaction. The root nodule bacteria provide very tempting support for the view that bacterial intracellular symbioses are common elsewhere, but it is possible that these legume nodules represent some very special case. The chlorellae also are quite persuasive. I got curious about those in *Paramecium*, and found that they have good, easily demonstrated nuclei, so they are the real thing.

The example of Carter's pineapple mealybug as one of cytoplasmic inheritance involving symbionts is more persuasive in summary than it deserves. In the original paper, it will be seen that he had to transfer very large numbers of pineapple bugs to the grass in order to get enough individuals back from the grass to work with; this obviously implies very strong selection. Also, the original pineapple bug colony was highly variable with respect to the morphology of the symbionts, while the recovered forms were not. Therefore, at least as far as the 1936 paper is concerned, the results could be interpreted as simple selection from an originally heterozygous mealy bug population, and the "symbionts" as cell particulates whose morphology is under control of the mealy bug's chromosomal genotype.

My own present hypothesis on the subject of endosymbiosis (intracellular) in insects is that these large "bacteroid" particles represent one or more kinds of mitochondria, that they are very widespread in insects, and that they are involved in some as yet unknown but very important phase of insect physiology. I do not think that function involves the production of some kind of vitamin; they are often massive in occurrence, and anyway, there is no reason for thinking that the insects having them do not get plenty of vitamins from their food and bacteria-filled guts.

I hope that this criticism is constructive. Probably these insect "bacteroids" are something outside the important problems you are dealing with, and that Kappa particles, rickettsias, bacteriophages, etc., are more to the point.

Sincerely yours,

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